

paper which many Englishmen have often the opportunity of seeing; and second, because the article in question occasioned considerable discussion.

With this reference to the relationship of my experiments to those of Mr. David Stevenson, and I will furnish others if required, I think it would only be just for Prof. Piazzi-Smyth and Mr. D. A. Stevenson to withdraw their accusation of July 2.

I must now refer to a second point which Mr. Stevenson has raised. In your issue of August 6 Mr. Stevenson appeals to the results of my experiments as showing the value of his aseismic tables. All my experiments with aseismic tables, whether used for seismographs or buildings, when approximating in form to Mr. Stevenson's joint were, for a variety of reasons, unsatisfactory. Under compulsion I have stated that Mr. Stevenson's joint itself, as applied to the lighthouses of Japan, has been unsatisfactory. I did so for the following reasons:—

(1) Shortly after erection the free motion of the tables occasioned so much inconvenience, that the European engineers then on the Japanese service had them clamped. For this reason Mr. Stevenson's arrangement was not adopted in lighthouses which were subsequently erected (see Brunton on "The Japan Lights," Institute of Civil Engineers, No. 1451, p. 9).

(2) I learn from the Lighthouse Department that in 1882, wishing to give Mr. Stevenson's tables another trial, several of them were put in working order. The result has been that on March 11, 1882, at Tsurigasaki, a number of lamp-glasses on the burners on the aseismic tables were overthrown.

Some time afterwards a second shock produced a similar effect. At neighbouring lighthouses, of which there are several (two being within 8 miles), not provided with aseismic tables, no damage was sustained.

The shock of March 11, 1882, which was felt for at least 300 miles along the coast, was severe, and its effects at Yokohama and Tokio, which are no great distance from Tsurigasaki, were carefully recorded. I am not aware that any small articles like lamp-glasses, bottles, vases, &c., were overthrown inside ordinary houses (see *Trans. Seis. Soc.*, vol. vii. part ii. pp. 41-44). The fact that no ill effects occurred at other lighthouses provided with Mr. Stevenson's tables, like those on the Inland Sea and near Kiushiu, must not be regarded as an argument in favour of the tables, inasmuch as the earthquake referred to was not felt in those districts.

I may here remark that several of the aseismic tables are at places where earthquakes are almost as rare as they are in Britain. Mr. Stevenson tells the readers of NATURE (June 26) that his lighthouses suffered when the aseismic tables were *not* in working order. I have shown that they suffered when they were in good working order.

Speaking generally about Mr. Stevenson's descriptions of his aseismic arrangement, he invariably refers to it as a great success. Where it was applied, earthquakes have produced no effect, but where it was suppressed, evil effects have resulted. After inquiries at the Lighthouse Department, which is a branch of my own department, I find that the facts adduced by Mr. Stevenson are exactly the reverse of the facts which have been placed at my disposal; and from what I learn, and from my own experiments, I conclude that thus far Mr. Stevenson's tables have been a failure.

As a further support to my conclusions I will quote the following translation of a report from the chief lightkeeper at the Tsurigasaki Lighthouse:—

"Sir,—On October 15, 1884, at 4.16 a.m., very severe shocks of earthquake were felt. The aseismic table was in working order, but the shocks were so violent that fifteen lamp-glasses out of twenty-one in use were upset and broken. The lamps thus stripped of glasses began to smoke. The milled heads of the wick-holders being shaken off, and besides the revolving machine being in motion, we had some difficulty in replacing the glasses promptly; however, we managed to put them all in proper order again by 4.21 a.m.—I am, Sir, your obedient servant, &c., &c."

The only form of aseismic joint that I have found at all practical is one where something more like a layer of cast-iron sand rather than a bed of cannon-balls is used to break the continuity between a structure and its foundations. I arrived at this after spending much time and, I may add, a considerable sum of money, and although the method involves the same principle as Mr. Stevenson's tables, I hardly think he is justified in claiming my successes to back up what I cannot but feel have been his failures.

If the ways and means permit, I hope to make experiments upon a small building resting on a bed of sand or fine gravel. Should results of any value be obtained, surely Mr. Stevenson will not expect me to do more than I have done already—namely, to state the relationship which may exist between these experiments and those which he carried out at the expense of the Japanese Government.

If everything connected with earthquake investigation which involves the same principles as are involved in Mr. Stevenson's lighthouse tables are to be regarded as his creations, he cannot avoid claiming the rolling sphere seismograph, the rolling cylinder seismograph, the horizontal pendulum seismograph, the conical pendulum seismograph, and in short, a very large proportion of the work accomplished by the Seismological Society of Japan. To this I cannot assent. All that Mr. Stevenson can be accredited with is a particular method of applying a principle, and this method has to my mind been a failure.

The question of the principle involved in Mr. Stevenson's device is one that has been repeatedly discussed in Japan. As an example of these discussions I will refer to the *Transactions* of the Seismological Society of Japan, vol. iii. p. 9, where Prof. J. A. Ewing is speaking, amongst other things, about a rolling sphere seismograph, the invention of Mr. Thomas Gray. Prof. Ewing says that Mr. Gray's contrivance was an application of "the method of supporting a mass by a movable piece in such a manner that the connection with the earth was at the centre of percussion of the movable piece, the mass being at the corresponding centre of instantaneous rotation, while at the same time the supporting piece was arranged so that its movements did not introduce any disturbing force due to the action of gravity upon the mass. This kinetic property, common to all these instruments, he believed he might fairly claim to have introduced into seismometry."

If Messrs. Stevenson and Smyth see fit to comment upon these notes, I trust that they will distinctly state whether they yet consider that I have attempted "to get round the letter" of Mr. David Stevenson's invention, and whether they were justified in publishing the objectionable personalities about "a B.A. man" on July 2.

This is the main point at issue, and if they choose to neglect it, the discussion may be considered as ended.

In conclusion I may remark that it was not I who commenced this controversy.

JOHN MILNE

Tokio, January 6

On the Velocity of Light as Determined by Foucault's Revolving Mirror

A FEW years ago Lord Rayleigh raised an interesting question as to the quantity actually determined by our experiments and observations on the velocity of light. There can be no difficulty as long as the medium transmits different wave-lengths with the same velocity, but whenever the medium possesses the property of dispersion the velocity with which any one crest of a wave travels (V) is different from the velocity with which a group of waves is propagated (U); hence the question arises in each particular case, whether it is V or U or something depending on both quantities that we measure.

In his first article on the subject (NATURE, vol. xxiv. p. 382) Lord Rayleigh states that, in experiments with Foucault's revolving mirror, the group-velocity, U , is determined; but subsequently (vol. xxv. p. 52) he corrected this statement and gave V^2/U as the quantity measured. A paragraph was added, however, in which the remark is made that, if a convex lens is interposed so that an image of the slit is formed on the fixed mirror, the rotation of wave-front, caused by the different velocities of different wave-lengths, and acquired on the outward journey, is neutralised during the return, so that in this case we should measure V .

Gouy (*Comptes rendus*, ci. p. 502, 1885) dissents from Lord Rayleigh's second view, and gives U as the quantity determined; without, however, giving sufficient reason in support of his opinion.

Finally, Michelson has performed the experiment with bisulphide of carbon, and obtained a result in close agreement with U . In a discussion of Michelson's measurements in the *American Journal of Science*, by J. W. G., his result is said to give "no countenance" to the theory which would make the velocity observed V^2/U .

It is the object of this communication to support Lord Ray-

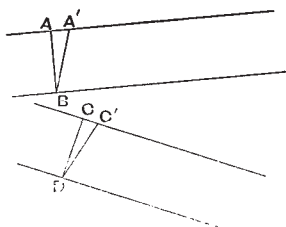
leigh's value V^2/U for the arrangement considered by him, but to point out that, if only one revolving mirror is used, the experiment cannot be performed in this way. The lens spoken of in Lord Rayleigh's second note is, then, a necessary condition of the experiment. When the lens is used it seems to me, however, that the result ought not to be a neutralisation of effects, but rather an equal rotation in the opposite direction. I have Lord Rayleigh's authority for stating that he agrees with this conclusion.

The method of Foucault's revolving mirror thus measures neither V nor U , nor V^2/U , but $V^2/(2V - U)$. As V and U differ by a small amount only, the last expression becomes nearly equal to U , so that Michelson's experiment is in complete agreement with theory.

In Foucault's experiment, it is well known, the displacement of an image is measured which is due to the rotation of a reflecting mirror in the time of passage of the light from the mirror to a fixed mirror and back again. But in order that this quantity should be capable of measurement, it is necessary that a displacement of the mirror should not by itself alone cause a displacement of the image.

The following consideration will show what conditions this imposes on the arrangement of the experiment.

In the accompanying figure let AB be the position of a wave-front as it leaves the mirror (supposed fixed). Let CD be the position as it returns to the mirror. I have drawn the wave-surfaces plane, for the sake of simplicity, but the argument remains the same if they are curved. The point A may correspond to the point C on the returning front or to the point D . If now the mirror is displaced through a small angle, the wave-front takes a different position, $A'B'$. If this displacement shall not change the position of the image after reflection from the mirror, the returning wave must turn through the same angle and take up the position $C'D$. But it is easily seen that, in order that this may be possible, the point A must correspond to D and B to C , for the optical length along any ray between two wave-



fronts remains the same; also, owing to the maximum-minimum property, we can still measure optical lengths in the displaced position along the original paths; and the length of the ray leaving A having been shortened by a distance AA' , the ray leaving B must be shortened by an equal amount, CC' —that is to say, the ray arriving at C must be the one leaving B , and the ray arriving at D must be the one which left A . We may then express the condition of a stationary image thus: The wave-surface must be inverted by the optical arrangement interposed, and the magnifying power of this optical arrangement must be equal to one. The last part of the condition is rendered necessary by the fact that the width of beam must remain the same; otherwise AA' could not be equal to CC' . If both conditions are fulfilled, the image will remain distinct in the rotating mirror, otherwise it would be drawn out into a band.

But an arrangement is possible, at any rate theoretically, in which the wave-surface is not inverted. We might have a second mirror rotating in such a way as to neutralise the effect of displacement. For this purpose the second mirror ought to rotate twice as fast as the first, the light being sent on to this second mirror on its way back only, after reflection from the first revolving mirror. As the angular velocity of the two mirrors must have their velocity accurately adjusted, the experiment would be difficult to perform, but it is important to point out that it is theoretically possible.

Returning now to Foucault's arrangement, consider two successive wave-fronts as they leave the revolving mirror. The distances between them will be larger on the receding than on the preceding side of the mirror. That part of the wave-front which is on the receding side will thus be propagated more quickly in a medium having dispersion, and whatever happens

to the wave-front on its journey, whether it is refracted, reflected, or inverted, that side of it which left the receding part of the rotating mirror will always gain on the other. When the wave-front returns to the revolving mirror, it will have rotated through the angle given in Lord Rayleigh's second note; but we have to consider the direction of rotation.

If that part of the wave-front which left the receding half of the mirror will return again to the receding half, the final rotation of wave-front will be in the same direction as the rotation of the mirror; and the displacement of the image which depends on the relative rotation of mirror and wave-front will be diminished. In this case, however, as I have shown, we want a second revolving mirror, otherwise the image of the slit will be drawn out into a band.

In the experiments hitherto performed, the wave-front is inverted an odd number of times between the two mirrors, and hence that part which left the receding side of the revolving mirror will now be on the preceding side. The relative rotation being increased, the observed displacement of the image will be increased.

The total observed rotation will thus, with Lord Rayleigh's notation, be

$$\frac{4Dw}{V} \left(1 + \frac{d \log V}{d \log \lambda} \right),$$

or the velocity of light calculated will be $V \left(1 + \frac{d \log V}{d \log \lambda} \right)$. As

$U = V \left(1 - \frac{d \log V}{d \log \lambda} \right)$, the apparent velocity becomes equal to $V^2/(2V - U)$. If $\frac{d \log V}{d \log \lambda}$ is small compared to unity, we may write

the expression for the apparent velocity approximately

$$V \left(1 - \frac{d \log V}{d \log \lambda} \right) = U.$$

In the case of bisulphide of carbon, J. W. G., in the passage quoted above, gives for the green rays $K/V = 1.637$, $K/U = 1.767$, K being the velocity *in vacuo*. From this we get for the theoretical value of the quantity observed in Michelson's experiment 1.758, being identical with the value 1.76 actually observed.

If a second rotating mirror is used, as described, Lord Rayleigh's value V^2/U remains true.

We have then as a final result, that, while the aberration of light measures V , and the eclipses of Jupiter's satellites and Fizeau's experiments measure U ; Foucault's revolving mirror measures either $V^2/(2V - U)$, or V^2/U .

ARTHUR SCHUSTER

Variable Stars

IN the last number of NATURE (p. 397) Prof. Seeliger, of Munich, is represented as thinking "it not improbable that the blazing forth of the *Nova* may have been due to a collision which caused an enormous development of heat and light." It appears to me that the collision hypothesis is not necessary, and that the variability of a star is a physico-chemical consequence of mere cooling. This conclusion is based on considerations relating to the formation of the chemical elements.

If we suppose a mass of primitive matter, say gaseous, to be cooling down, it will from a chemical point of view undergo a series of changes such as are indicated in the known transformations I to I_2 , NO_2 to N_2O_4 , &c.; in short, it will produce a succession of polymers. When, however, each stage of polymerisation is exactly reached, the formation of each new polymer being attended with an increase of density, will, from a physical point of view, lead to an evolution of heat. This evolution of heat will occur periodically as polymerisation goes on; and if the heat be sufficiently intense, there will be a corresponding periodic development of light. The number of such periods will doubtless depend on the nature of the primitive matter, and the difference between its temperature and that of its environment. I cannot recall an instance where, in the laboratory, more than three polymeric periods have been attained; but celestial events have a wider range and greater possibilities.

The evolution of heat which necessarily occurs at the point of polymerisation involves of course a partial reversal of the polymerisation. Elements therefore will be formed which may be regarded as derived from successive polymers by virtue of such reversal.